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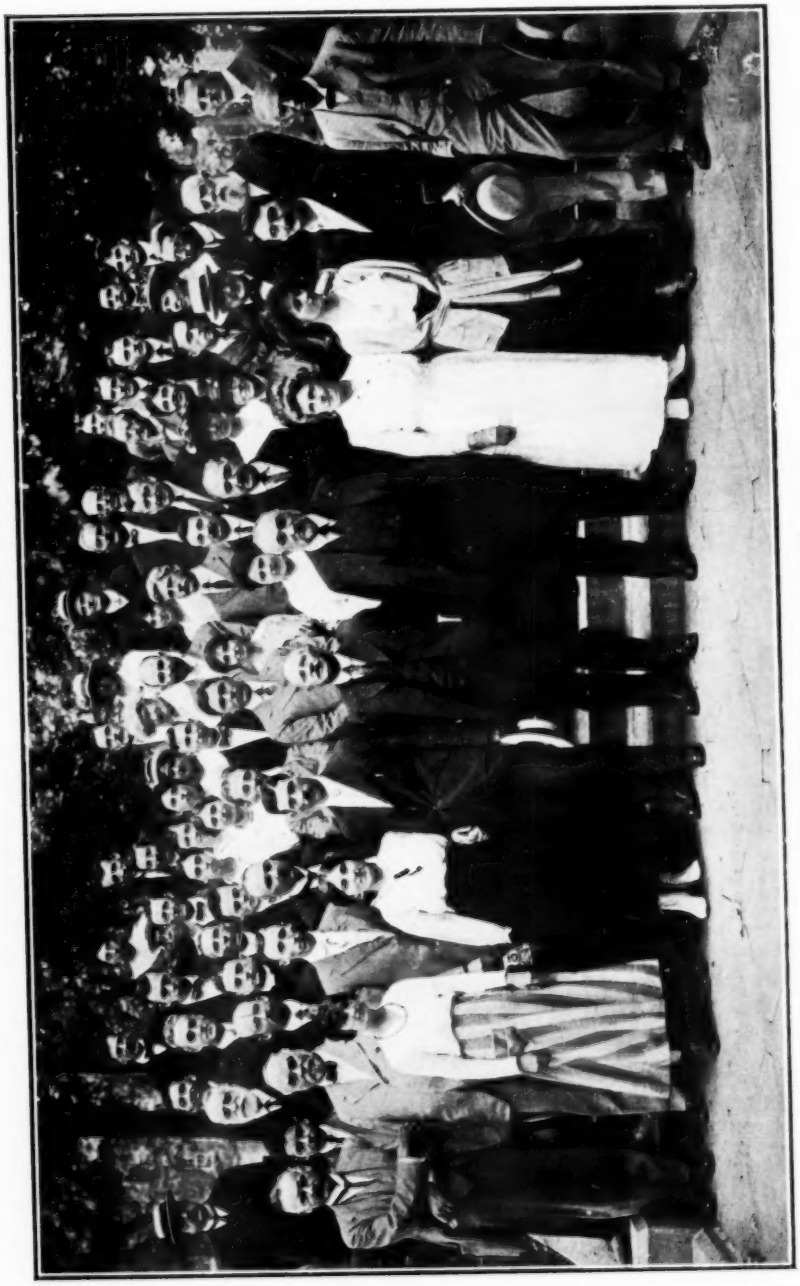

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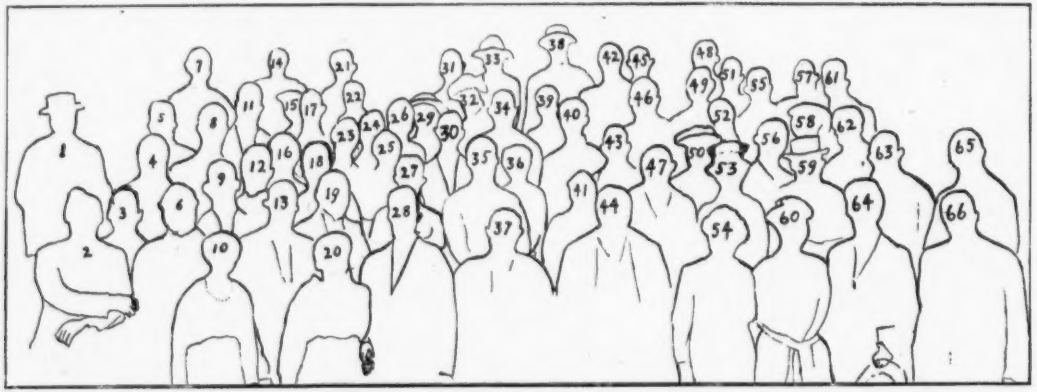

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THE PLANETESIMAL HYPOTHESIS*

BY T. C. CHAMBERLIN

TO those who feel an interest in the evolution of the earth, it is of first moment to learn how the planet was born and what conditions controlled its growth. The other planets of our system no doubt had a birth of like nature and these, as well as the earth, bear traits that serve as signs of the family lineage. It is not clear that the sun was born at the same time, or in just the same way. There is no doubt that the sun and the planets are close akin but this does not make it sure that they were participants in a common birth. We shall present evidence that the planets sprang from the sun, not at his birth, but later in the course of his history. The satellites might easily seem to be the offspring of the planets, and this was the common view in the last century, but there are signs that planets and satellites

*On June 2, 1916, an important book was published by the University of Chicago Press, entitled *The Origin of the Earth*, the author being Thomas Chrowder Chamberlin. (xii + 272 pages, price $1.50.) The article printed here appeared in the international journal "Scientia" in 1914. It gives in concise form the theory elaborated in the larger work.—Editor.

had a common birth and that the satellites escaped being little planets only because their birth-places fell within the spheres of control of their larger sisters to whom they were forced to dance attendance as a first duty, and respond to the common call of the sun incidentally. If these relations are true, we may search the planets, planetoids and satellites for signs of the later family history, while we look to the sun for signs of their parentage and of the earlier history.

But are these the real kinships? Let us look to the evidence.

If the planets were separated by centrifugal action from a nebula of which the sun is regarded as the residual mass—the view that was foremost during the last century—the planets should revolve in paths that lie in the plane of the sun's equator. But the plane of the earth's orbit is inclined 7° 15′ to it and the planes of the orbits of the other planets and planetoids diverge at other angles. These angles may not seem to imply a very wide departure from the theoretical requirement, but when the immensity of the momenta of these bodies is considered the divergence is really serious.

But there are much more formidable difficulties. If the planets were separated from the parent nebula at successive stages by centrifugal action, the equatorial rotation of the nebula must have had successively the speeds of the separated parts. These speeds may indeed have been slightly altered subsquently by tidal and other actions, but Sir George Darwin has shown how trivial the tidal effects must have been at the most, and the others are negligible under this hypothesis. The velocity at the equator of the nebula must have increased from about 5.5 kilometres per second at the separation of Neptune to more than 45 kilometers per second at the separation of Mercury. Moreover its velocity must have continued to increase still further as the nebula shrank into the existing sun. As this further increase must have much exceeded that which had already taken place, it might well be supposed that material for other planets would have been separated, and this view was entertained in the last century and diligent

search made for inner planets at times of solar eclipses; indeed eminent astronomers even announced the discovery of such planets, but the observations proved illusory. If the radius of the orbit of such a planet had been 1,612,900 kilometers its orbital velocity would have been 274 kilometers and the equatorial velocity of the nebula should have been the same at the time of its separation. To separate matter by centrifugal action at the equator of the present sun, the velocity should be 435 kilometers per second. The actual velocity is about 2 kilometers per second. This is a discrepancy so enormous as to seem fatal to the centrifugal theory.

Moreover this difficulty is supported by others not less formidable, though time will not permit us to consider them adequately. Suffice it to say that the values of the planetary momenta seem to be seriously at variance with the requirements of the centrifugal theory. Attention was directed to some of these troublesome features by Babinet a half century ago but he does not seem to have regarded them as fatal, but only as limiting conditions to which the theory must conform. Much later and quite independently Moulton carefully inquired into the question whether the moments of momentum of the solar system were compatible with the centrifugal theory and found the discrepancies insurmountable. As his arguments rest upon the constancy of moment of momentum, a firmly established principle, they are scarcely less than rigorous.

A less technical argument may be appreciated by laymen, who may readily picture to themselves the solar nebula just before the matter of Jupiter and his satellites was separated from it according to this hypothesis. The mass of the Jovian matter was less than one-thousandth part of the mass of the nebula at that stage. A rough estimate may easily be made of the momentum that could be carried by this little outer rim of one-thousandth part compared with that carried by the remaining 999 parts. It is obvious from even a mere inspection that the moment of momentum of the thousandth part could only be some minor por-

tion of the whole. But yet the Jovian system actually has more than nineteen times as much moment of momentum as all the inner bodies, including the sun, which were supposed to have been formed from the rest of the nebula. It seems incredible that the material for the Jovian system could have been separated in this way. A like inspection of other supposed centrifugal separations leads to analogous discrepancies, some of them proportionately more remarkable.

The cumulative force of the objections to the centrifugal hypothesis seems too grave to be escaped by any special pleading. This does not mean that centrifugal separation cannot give rise to planetary matter in any case, but merely that origin by such separation does not fit the requirements of our present planetary system.

The satellies also offer a silent protest against the pedigree assigned them by the centrifugal hypothesis. The orbital speeds of Phobos and of the small bodies that form the inner ring of Saturn demur. They cannot consistently concur in the implications of the centrifugal pedigree. Particularly sharp is the dissent of two satellites recently found to revolve in directions not only contrary to the postulates of the hypothesis but opposite to their fellow satellites attending the same planets.

In the face of these serious difficulties—not to cite others—there seems to me no alternative but to abandon the hypothesis that our present planets and the sun were formed in close succession from a common nebula by centrifugal separation. Some other mode of origin must be found that better tallies with the significant facts of the system. Obviously it must also tally with the great events of cosmic evolution of which the origin of our little system was but a trivial part, however important to us. But before considering these more general relations, let us look to the terms of the hypothesis we offer to better fit the facts of the case.

It has been implied already that the genesis of our sun—and of course the genesis of stars in general—is not regarded as a part of our immediate task. The evolution of the sun is held to

be an earlier event. The birth of the present planetary system is assigned to an intercurrent incident, a later episode in the sun's history. This is one of the distinctive features of the new hypothesis. This later genesis of the planets seems required because the rotational features of the sun and the revolutionary features of the planets are so out of accord as to imply origins under different conditions. This is equally implied by the discordant ratio of mass to momentum. The sun holds about 745 out of 746 parts of the total matter of the solar system, while it only carries about 2 per cent. of its moment of momentum. These discordant features lead to the conviction that a new agency came in, after the original formation of the sun, and gave to a very small fraction of the solar matter—after it had been drawn out from the sun—a special endowment of momontum. This action is assigned to so simple an event as the passing of a star (or other massive body) near the sun, calling forth a very small fraction of his mass and endowing it richly with momentum from the visitor's own store. As there are a hundred million or more stars in the heavens moving in diverse directions and at varying rates, much like the molecules of a gas, many close approaches and some collisions may be regarded as inevitable, and so our appeal is made to an event of high probability. Let it be noted that no appeal is made just here to actual collisions of stars but merely to approaches of such degrees that there must have been millions more of them than of actual collisions.

The mode of action of the passing star in calling forth a trivial fraction of the sun's substance and giving it high momentum in orbital form, rests upon an extension and adaptation of a principle enunciated long ago by Roche and confirmed by Maxwell and others. It was shown by Roche that if a secondary body were made to approach its primary, it would be torn asunder by the differential attraction of the primary body so soon as it reached a point 2.44 times the radius of the primary, provided both were homogeneous bodies of the same density, and provided cohesion, elasticity and all expansive agencies are neglected. The

principle holds in other cases of close approach than the special one studied by Roche.

Now, if the Roche principle be extended to bodies affected by a strongly expansive and even explosive habit, as in the case of the sun, it is obvious that special effects must be predicated. The influence of the attraction of a passing star is of the differential kind made familiar by the tides. The differential pull of the passing star draws out the explosive body along the line joining it and the star, facilitating explosion along this line. At the same time it compresses the explosive body at right angles to this line, and so localizes and intensifies the explosive action, Thus the explosive body may be said to have become a Janus-faced piece of ordnance belted and compressed about the middle while it fires its projectiles toward and from the passing star. The projectiles shot toward the star are not drawn toward it by its attraction during their flight, but are drawn *forward* by it in the direction of its own movement. The projectiles shot in the opposite direction suffer an opposite effect in pursuance of well known tidal laws. If the projectiles during their flight are sufficiently deviated by the passing star, they will assume elliptic, parabolic, or hyperbolic orbits instead of returning to the sun as they would do in the absence of the deviating effects of the star.

If the forward pull of the passing star is relatively small, as is likely to be the case when the star is distant or small and the ejection from the sun is consequently short, the projectile will fall back to the sun, but it will carry back whatever momentum it gained from the forward pull of the passing star and this will accelerate or retard the rotation of the sun according to the relative direction of the sun's rotation at the time. If the forward pull has a deviating effect greater than the radius of the sun, the projectile will fall into an elliptical orbit about the sun; the only exceptions being the cases in which the deviating effect is so great that it causes the projectile to pass into a parabolic or hyperbolic orbit and be lost, or else into a reversed elliptical orbit about the passing star and thus become secondary to it. All

these cases are not only possible, but all these cases were actually encountered by Dr. Moulton in tracing out the courses of the projectiles in the first nine test cases which he followed mathematically to see the actual workings of this combination of agencies. In these cases he made no postulates relative to the mass, position, orbit or velocity of the passing star, or of the projectiles from the sun, that did not seem to lie within the probabilities of the case. In forty-eight cases later tried, he found the dispersing potency of the combination surprisingly effective, even when the passing star was not more massive than our sun, and its approach not usually as close as that of the earth to the sun, though that distance and half that distance were used in a few cases. The remarkable fact was revealed that gravitation, commonly supposed to be the supreme agency of celestial concentration, is really, under the conditions of swiftly approaching bodies of explosive habits, a very effective agency of dispersion.

It was further shown that the passing star may remain within an effective range of action for a period of twenty years in certain cases. The average period in the cases selected proved to be about five years.

During the effective period of influence a succession of explosive impulses must almost certainly take place, and these should give a stream of projectiles in the form of bunches or pulses of solar matter attended by diffusely scattered matter, the two elements constituting, by interpretation, the knots and haze so common in spiral nebulæ. These streams would of course issue on opposite sides of the sun and would curve in opposite directions, a distinctive feature also common in spiral nebulæ.

It is not difficult to see that these features would arise naturally, if not inevitably, under these conditions, but it taxes the imagination somewhat more severely to follow the composite action through the whole series of pulsations called forth successively by the disturbing star until it passes to an ineffective distance, but if done, it will appear that the arrangement of pulses at any instant takes the form of a spiral with two streams

issuing opposite one another and consisting of knots and haze, the whole effected with much irregularity but retaining a general symmetry. From the nature of their origin, the streams lie nearly in the plane of the orbit of the disturbing star, and hence the whole takes in a disk-like arrangement. When seen on edge the outline is narrowly elliptical. This discoidal form is well suited to give rise to a group of planets arranged also in discoidal form as is the case with aur planetary system.

The spiral feature, it is to be noted, relates to the *streams* of knots and haze, and not to the *individual paths* of each separate constituent. These paths are held to be elliptical, in the main, and to be controlled by the centre of mass of the spiral. The knots must inevitably be more or less rotatory, and when large they must control the immediate movements of the matter within their spheres of influence, and so there should be revolutions about subcentres subordinate to the general revolution about the centre of the nebula.

The spiral form cannot last indefinitely. If the central mass is large and the dispersion only moderate, the inner parts must revolve much faster than the outer ones, and so the spiral streams must wrap up, growing more and more involute till they merge into a disk and thus take on one of the forms of planetary nebulæ. In some cases the spiral must merge into a disk in a short period. But if the dispersion be great and little matter be left in the central part, the differences in the rates of revolution of the several scattered parts may become small and the spiral wrap up at an exceedingly slow rate and hence its endurance be long.

The mathematical tests of Dr. Moulton clearly implied that for the evolution of a spiral nebula suited to the formation of the present solar planets, the approach of the disturbing star must have been rather distant, for the explosive ejections amounted only to an extremely small part of the whole and the dispersion was relatively limited. Really close approaches of massive stars when one or both have highly explosive habits and when the velocity of approach is exceedingly high and the path at the

critical stage is a sharp curve, must produce effects of a higher order, and give rise to spiral nebulæ of a much larger and more dispersed type. Consider, for example, the case of a star of the smaller order passing through the Roche limit of a star of the more massive order. Let the stars have about the mean velocities of stars of the mature type before they appreciably affect one another. As the stars approach one another these velocities will rise to such a degree that when the smaller star is passing through the Roche limit of the larger star its velocity may reach some few hundred kilometers per second—depending upon the masses of the stars—without making the case an unusual one. Neglecting dispersion for the moment, the smaller star must swing around the larger star in a sharp conic curve and all the while be subject to extreme differences of gravitative pull from the larger star. Under the Roche principle these differences would be sufficient to wrench the smaller star into fragments if it were merely a passive body held together by its own gravity; i.e., its self-gravity would be somewhat more than neutralized. It follows that in the process all the expansive potency of the star is set free for dispersional work. But this is not done promiscuously nor instantaneously. The star, during its approach, has suffered tidal elongation, giving rise to tidal cones fore and aft and to compression about the middle portion. The cones have grown higher and sharper, and the middle has become more and more compressed as the approach grows closer, so that when the Roche limit is entered the star is rather a fusiform bolt than a sphere. It is moreover constantly undergoing torsion by the changing direction of the mutual pull of the two stars. The tidal cones also are constantly lagging because a gaseous body is viscous. While therefore we may say that the total explosive force of the small star has been set free by the time it has entered the Roche limit, the explosive action has all the while been directed fore and aft by the differential way in which the gravity of the large star has acted. Since the tidal cones—the two muzzles of our gigantic piece of ordnance—constantly lag, and since the curve about the larger star is sharp,

the projectiles are never shot directly at or from the larger star, but at a point in its rear and at a point opposite this. The ordnance itself however is on a rapid swing and the successive projectiles take changed paths. This may help the layman to see how the projectiles successively shot forth during the swing will be related to one another at any instant. Theoretically, under these conditions, about all the substance of the smaller star will be shot away in the form of projectiles. The ordnance is at once missile and ammunition. Only the necessary incompleteness of the theoretical action leaves matter behind to form the nucleus of the resulting spiral. Spiral nebulæ with small nuclei are occasionally seen in the heavens. Of course they should be relatively rare,, for a passage through the Roche limit cannot often take place, though it is half dozen times as likely to happen as a collision.

While the picture of the fusiform bolt is fresh in mind, it may be observed that grazing approaches and glancing collisions are not instances of the touch or the overlap of spheres pursuing straight lines, as illustrations too often imply, but of the touch or the overlap of two elongated slightly curved fusiform bolts swinging violently about their common centre of gravity on sharp conic curves.

Collisions must in the nature of the case be rare compared with dispersive approaches of the different types. They need only be mentioned here to fill out the series. A glancing collision is the next step beyond a grazing approach and must quite surely give a spiraloidal dispersion. A cente-to-centre collision must give a radial or irregular dispersion of extreme violence.

The spiralizing whirl given to an assemblage of matter in swinging about another assemblage is probably not confined to continuous bodies like the stars but applies also to clusters or galaxies of stars when these closely approach and swing about one another. It probably applies even more effectively when the clusters pass through one another on curved paths. And so, if the view that some of the greatest of the spiral nebulæ are only extremely distant assemblages of stars, a view not without serious

objections, should yet prove true, it would perhaps only be a higher expression of the workings of the principle of disturbing and distorting approach. It is to be borne in mind that assemblages about common centres of gravity, whether continuously or disjunctively organized, do not pass one another on straight lines and at even pace, but bend their courses into elliptic, parabolic, or hyperbolic paths and greatly hasten their speeds, while differential attractions arise and increase with approach and inevitably impress their own peculiar qualities on the conjoint action.

In assigning most spiral nebulæ to the incidental effects of the close approach of stars, our hypothesis tallies well with the fact that the number of spiral nebulæ greatly exceeds that of any other form. In announcing their great preponderance, Keeler remarked that they must be due to some cause of common occurrence in the heavens. In view of the multitude of stars, the diversity of their courses and the variety of their velocities, it is difficult to assign an occurrence of a highly potential kind that is likely to be more frequent than the close approach of stars to one another. But sufficient frequency is the least significant part of the coincidence in this case, for, to meet the requirements, the occurrences must not only have had sufficient frequency but have had such a nature as to give these peculiar results, 1. a spiral form implying at once radial and tangenital action, 2. a pulsatory effect, shown in streams of knots and diffuse matter, and 3. a double out-streaming from the centre, taking the form of two arms issuing from opposite sides and branching diffusely in their distal parts. The heavens do not present a more distinctive deployment than the spiral nebula, and yet it is one of singular frequency, the dominant species of the nebular type. An ecentric collision is no doubt competent to produce all grades and types of spiral nebulæ, or even the majority of the forms, while the probability of its occurrence is relatively small. Even so far as it is competent and adequate, it appeals to essentially the same principles of action as those that actuate dispersion in cases of approach. It thus appears to be only the last term in a long series, the less catas-

trophic members of which constitute the majority of actual cases. Eccentric collisions seem too rare events to give origin to so dominant and so varied a group as the spiral nebulæ. They are therefore regarded as one source of spiral nebulæ but not an adequate source. Disturbing approach may have a numerical competency from one million to ten million times as great as actual collision, and yet it is not clear that it has any surplusage of competency to keep up the supply when the evanescent nature of the smaller nebulæ is taken into account. This is on the assumption that the present supply of nebulæ is to be maintained by the present galaxy of stars in their normal intermovements. If one great cloud of stars is now passing through another, as Kapteyn has suggested, the number of recent dispersive encounters may have been exceptionally large and the present number of spiral nebulæ abnormally great.

In thus following out in some detail our conception of the origin of spiral nebulæ, we have wandered far afield from the planetary problem with which we started; let us return to the modest nebula that was drawn out, by hypothesis, from our sun and was destined to form the family of planets on one of which we dwell. As already implied, the material was sun-substance at the start, and of course in a gaseous state for the greater part, if not altogether. This gaseous matter was shot out in successive pulses by recurrent explosive action, and these pulses took the form of irregular knots attended by much highly scattered matter.

Pulsatory action in a persistently explosive body like the sun seems to be a normal habit and is well illustrated by the sun's present action in projecting "prominences." Such action is likely to become rhythmical if there are appropriate agencies. Such agencies may be assigned in the formation of the solar nebula. Every spheroid has a period of pulsation that is normal to it, and this period is quite certain to influence the periodicity of any impulsive agency that widely affects the body. Nagoaka holds that the successive explosions of Krakatoa during its catastrophic action in 1883 substantially coincided with the pulsation-

period normal to the earth. The pulsation-period of the ancestral sun may be assumed to have influenced the succession of explosions made imminent by the increasing influence of the passing star.

There was perhaps a more influential factor. It is assumed that the plane of the orbit of the passing star crossed the plane of the ancestral sun's equator obliquely, for this seems to be implied by the present obliquity of the sun's equator to the invariable plane of planetary system, and also by the anomalously low rotation of the sun which probably implies a reversal of the sun's ancestral rotation. When the tidal cones developed by the star crossed the equatorial belt of the sun, they came into coincidence with its explosive belt and the effect was intensified by this coincidence. It was also affected by the superior centrifugal components of rotation that affected this belt. The concurrent explosive action that arose at this time should therefore have been exceptionally effective. This concurrence probably also fell in with a more or less perihelion position of the star at which time its differential effect was greatest. This concurrence may be the special reason for the greater size and distance of the major planets.

unbalanced elements in the ejective impulses naturally gave rotation to the knots, and this was united with whatever rotatory property they already had as parts of the sun, so that there was a beginning of rotation at the very start. The large knots necessarily influenced the movements of the smaller knots that were shot out with them and kept near them, indeed they fully controlled any that remained within their "sphere of influence" in the technical sense. The whirl of each bunch of knots sent forth by a given explosion would naturally tend to relate itself to a common plane and the knots would generally have a common direction of revolution but reversals in some cases might obviously arise. Herein lies the basis of origin of the satellites.

On emerging from the sun, the ejected matter suffered great expansion and consequent cooling, and this was followed by fur-

ther cooling as it traversed interstellar space. Much of the material should therefore soon have been reduced from its original gaseous state to the liquid and solid states; particularly must this have been true of the more refractory material which makes up the greater part of the earth and probably of the other planets. The highly scattered matter of the nebular haze could hardly have remained long in any other state than solid or liquid, but it was of course at first in minute division. As the temperature range of the liquid state is small, we may call the cooled matter solid, for convenience. The knots may have retained the gaseous state in larger degree, but the irregular forms they present in so many cases seem better to tally with the view that they too were made up largely of minutely divided solid matter in orbital motion. Doubtless gaseous matter formed some appreciable part of the knots, while a multitude of isolated molecules outside the knots doubtless pursued independent orbital courses about them.

The nebula is thus conceived to have been formed in the main of molecules and minute bodies pursuing orbital courses about the solar center, and subordinately about the gravity-centers of the knots. The movement of these minute bodies was of the same type as that of the planetoids and planets and hence they have been called planetesimals, and the theory of which they are the distinctive working element, the planetesimal hypothesis. Those minute bodies that, in addition to revolving about the solar center, revolved also about the gravity-centers of the knots, may be called satellitesimals if one wants to be specially precise, but the term planetesimal is the generic one and covers the whole class of small bodies revolving in orbits similar to those of the planets.

We have now the working mechanism for the remainder of the evolution. All the rest follows as a matter of celestial mechanics, and the details need not be pursued here. The knots, the natural centers of growth, gathered in the haze and grew to planets, planetoids or satellites. The eccentricities, obliquities and irregularities of the original planetesimal orbits involved many crossings and thus facilitated accretion. The shiftings of

the orbits by the mutual attractions within the system led on constantly to new relations and further aided aggregation until most of the planetesimals were gathered into the growing planets, planetoids and satellites. Perhaps some planetesimals remain unassembled and contribute to the phenomena of the Zodiacal Light and the Gegenschein. The combination of a multitude of bodies in somewhat eccentric and irregular orbits led inevitably to fewer and more circular orbits. The nuclei that grew most came to have the most circular orbits as an obvious consequence, and this is well exemplified in the present planetary system. The planetoids are especially eccentric, while the small planets, Mercury and Mars, have more eccentric orbits than the great planets. As a mechanical effect, each nucleus moved into a larger or a smaller orbit in proportion as more planetesimals of larger or smaller orbits were added to it, so that the growing planets tended to space themselves out automatically towards the less occupied feeding grounds. Probably the distribution of mass in the nebula gave rise to stable and unstable zones, and the planetesimal orbits were more or less bunched in the stable zones on account of this and thus also the growth and position of the planetary nuclei were influenced. Bode's law thus perhaps comes to have a physical meaning.

But about the directions of rotations of the planets! "Aye, there's the rub!"

At the time this hypothesis was first entertained, it had been, for a long time, a standard doctrine that planets formed from coherent rings would rotate in the direction of their revolution, that is forward, while planets formed from bodies moving in independent orbits would have retrograde rotations. If this law holds, our planetesimal scheme of growth, well as it seems to work in so many particulars, fails seriously here, for six of the eight planets have forward rotations. The two others rotate obliquely backward, at least their satellites revolve in this way. These last make trouble for the ring hypothesis, to be sure, but still it has greatly the advantage over the planetesimal hypothesis *if* the reasoning

back of the alleged law is sound and applicable. It runs in this wise:

In bodies that rotate as a unit, like a ring, the outer part moves faster than the inner part, and besides, every portion of the outer part goes once around the inner parts in every rotation of the ring. If therefore the ring is made to collect into a spheroid in any normal way, the spheroid should inherit a forward rotation. On the other hand, if a ring-like belt is made up of small bodies revolving in independent orbits, as do the particles that make up the rings of Saturn, the inner bodies must move faster than the outer ones, and if these bodies are aggregated in a normal way, it was held the resulting rotation must be retrograde. Here then there seems to be a lion in the way of all orbital hypothesis, and the planetesimal hypothesis is a most declared type of this class.

But is the reasoning applicable? In the case on which the reasoning is based, the orbits are circular. The whole line of reasoning, as well the ring hypothesis itself, seems clearly to have had its initial suggestion in the rings of Saturn, and very naturally so, as they seemed to be remnants of the process of evolution providentially left for our instruction. Roche and Maxwell, however, showed on theoretical grounds that they teach something very different, and Keeler showed by the spectroscope that Saturn's rings are formed of separate solid bodies and not of gases as was assumed in the Laplacian hypothesis. The orbits of the particles that make up these rings are nearly circular and if massed by some systematic process into single bodies these might not unlikely have retrograde rotations; that would depend however on the precise way in which the particles were brought together. But we need not dwell on this case for it is exceptional; the Saturnian rings were developed under the conditions postulated by Roche and are a peculiar feature. Most orbits in the heavens are not circular and strictly concentric, as are these, but are elliptical, and the planetesimal orbits were by hypothesis notably elliptical Now the velocity of a body in an inner elliptical orbit is indeed on the average higher than that of one in an outer elliptical orbit

of the same type, just as in the case of circular orbits, but *at the points* where an inner elliptical orbit cuts an outer orbit, and where alone the bodies in these orbits can come tigether, the velocity of the body in the *outer* orbit is higher than that of the body in the inner orbit, *precisely reversing the application of the law* This may be demonstrated mathematically, but the layman may prefer to visualize it. This may be done in the simpler cases. If a notably smaller elliptical orbit is placed concentrically within a larger orbit of the same type, there can be no collision. It is only when the major axes are so moved that a more or less aphelion or distal portion of the smaller oribt is made to coincide with a more or less perihelion or proximate portion of the larger orbit that collision can occur. If the dimensions be so selected that the precise aphelion point of the inner orbit can just touch the outer orbit at its perihelion point, it is easy to see that from this point the body in the inner orbit falls back in its outward course little by little toward the central body because its velocity is insufficient to maintain its perihelion distance, i.e., the common distance of the two bodies when they were together. If the orbits appreciably cut one another, the inspection reveals similar relations of the two velocities but less clearly, and in other cases mathematical demonnstration may be the only recourse. That will show, however, that this relation holds very generally but not universally.

The actual rotatory effects of the union of the two bodies in elliptical orbits vary with the precise conditions of their union. Keeping in mind that the body in the larger orbit moves the faster at the critical point, it is clear that if a collision occurs when the body in the smaller orbit is *approaching* the junction, the rotational effect will be forward, but if the body in the inner orbit has *passed* the junction, the effect will be the opposite. In a large number of cases both phases are quite sure to occur. The total effect when a multitude of planetesimals are added to a knot will be determined by the mean effect of all. The areas within which collisions tend to forward rotation are greater than the areas of

the opposite class, and thus the probabilities distinctly favor forward rotation, but irregular or special distributions make possible retrograde or oblique rotations.

Quite as important as the direction of rotation is the fact that the velocity of rotation is not measured by the simple sum of collisional effects of like sign, but rather the algebraic sum of effects of opposite signs, and so the rotation may be low or high according to the proportions of the opposing phases of collision.

Now these features fit the case in hand, for the velocities, axes, and directions of rotation of the planets are quite various and do not conform to the systematic requirements of concentration from a common source. Rotations, slow or fast, forward, backward or oblique, with axes differently inclined, are all consistent with concentration from orbits such as are postulated by the planetesimal hypothesis. There was indeed a lion in the way, but he was chained to a very special and exceptional case.

Perhaps the most important test that can be brought to bear upon theories of the origin of the planets is found in the planets themselves, the ultimate product; in our earth in particular, because it is accessible to close inspection. The Laplacian and the planetesimal hypotheses have been carried down to their specific planetary applications and have offered us definite stories of the early stages of the earth. These stages form the first chapter of earth history. The harmony of the stories with the later events of the self-recorded history bear critically on the verity of these stories of earth genesis.

The series of Laplacian pictures is very familiar: 1. a globe of gas; 2. a globe of white hot lava enshrouded in a vast atmosphere; 3. a globe crusted over and covered by a nearly universal ocean overhung by a still deep moist atmosphere; and then 4. a series of slow internal and external changes. Having grown thus gradually out of the fluid state, the concentric arrangement of material normal to fluidity should have dominated the whole evolution and have had marked expression in the final product presented for inspection and verification today

The series of planetesimal pictures depart widely from these. A nebular knot formed a nucleus at the start, partly gaseous, partly orbital, amounting, it may be, to a third or a half of the final mass. An early concentration of the knot into an earth-nucleus was followed by a very slow growth from the scattered planetesimals afterwards. The long series of infalls of planetesimals generated much heat, but chiefly in the upper zones of the growing atmosphere, whence it was easily and promptly radiated away. The magnetic and inelastic material was brought in faster than the non-magnetic and non-elastic, because magnetic attraction supplemented gravitative attraction, and because the orbital motions of the inelastic planetesimals were faster reduced by mutual collisions than by those of the elastic planetesimals. And so the metals and the basic rock-material gathered more largely toward the center, while the more elastic material gathered later and more largely into the outer parts. The accessions were very heterogeneous notwithstanding. The planetesimals plunging into the atmosphere became ignited by the stroke and were largely dissipated to dust which floated at the will of the winds until gravity or precipitation brought it to the ground or the sea. This flotation had a more or less sifting effect, separating, in some little degree, the heavier from the lighter material, thus building into the very body of the earth a differentiation of specific gravity. This differentiation was abetted by the growing hydrosphere, and both atmosphere and hydrosphere were localized in their activities by the increasing deformations of the earth-body. These united processes led thus on to continental embossments of lighter materials, and to abysmal sags of heavier materials. Radioactive matter, lodging at first promiscuously in the heterogeneous mass of the growing body formed a multitude of self-heating centers. These, cooperating with other sources of heat arising variously in the self-compressing body, started local liquefaction in such material as was specially fusible or soluble, and these liquid parts, under the kneading strains of the earth, worked outward and initiated volcanic action, the radioactive matter partaking in the

ascension and thus concentrating toward the surface. Life conditions arose early, and no doubt life soon followed and participated in the processes of growth. Thus slowly the earth grew to maturity and merged into the later history recorded in the known strata. The whole may be shortly summarized as a slow-growing earth-body, bearing a slow-growing hydrosphere, enwrapped in a slow-growing atmosphere.

Critical comparison of these two series of pictures must be left to special students of the earth and to time. Certain vital features of recent determination, which will largely control future opinion relative to the genesis of the earth, are worthy of special attention: 1. the undeniable evidence of an alternation of cold and warm climatic stages following one another from the earliest date to which climatic evidences reach, now raised to demonstration by a long list of competent observers; 2. decisive evidence of the deep differentiation of the specific gravity of the earth-material, brought out by Hayford's analysis of geodetic data; 3. evidence of the concentration of radioactive material in the outer part of the earth, brought out by Strutt, Joly, Holmes and others; and 4. the determination of the earth's high rigidity, initiated by Kelvin and others, supported by seismic data gathered by a host of observers, supported also by the pendulum studies of Hecker, and now brought to a full demonstration by the brilliant cooperative work of Michelson, Gale and Moulton on new lines. All these new developments are welcome from the viewpoint of the planetesimal hypothesis. Whether the terms of the older hypotheses can be revised to fit them or not, it does not fall to me to say.

The foregoing relates to the origin of our present planets and satellites. A few words may be added relative to other forms of planetesimal genesis, and to the origin of the comets and meteorites. Elsewhere I have endeavored to show that, on the surface of a great rotating body of hot gases, the molecular activities necessarily give rise to krenal flights of molecules, i.e. elliptical excursions outward from the body, which are checked by gravity and returned to it without the collisions that usually

terminate molecular flights within a body of gas. These krenal flights are especially promoted in the equatorial belt when centrifugal separation is imminent. Occasional collisions are inevitable in the course of the krenal flights, and some of these flights are thereby converted into orbital flights. Thus the dominant types of molecular flights are 1. *within* the atmosphere, intercollisional, 2. at the*summit* of the collisional atmosphere, krenal, and 3. in the *outermost* zone, orbital. The orbital atmosphere can only be formed from those molecules that have exceptional velocities, and hence its molecules carry exceptional energy and moment of momentum. The transfer of these exceptionally endowed molecules from the collisional atmosphere to the orbital atmosphere makes relatively heavy drafts on the energy and moment of momentum of the rotating spheroid. The transfer is accelerated by every acceleration of rotation, and the draft of energy and moment of momentum is thus accelerated as the critical point of centrifugal separation is approached. This accelerated draft seems to be such that the equatorial velocity never quite reaches the stage at which separation *en masse*, or even partitively, can take place by simple centrifugal action, but only by this indirect mode. The molecules that pass thus individually into orbits have paths that vary much from one another and are scattered widely throughout the sphere of control of the spheroid. They do not form a simple ring close about the shrinking spheroid, as in the familiar Laplacian conception, nor do they follow closely the analogy of the small bodies that make up the Saturnian rings. They constitute a variety of planetesimals, with elliptical orbits promiscuously crossing one another, and subject to aggregation in much the same way as the planetesimals of the spiral nebulae, save that there are here no nebulous knots to serve as collective nuclei. This last is a vital point, for, in the absence of collecting nuclei, the aggregates are likely to be many and small, rather than few and large as in our planetary system. The planetary family thus separated from a gaseous nebula in the course of its condensation should consist of a multitude of small planetoids of eccentric orbits and rather

diverse planes. A family of this type, on the near approach of a star at some later time, might be thrown into inextricable confusion and its members be liable to pass through the Roche limit of some other body of their own system, or of the new system, and to be torn into fragments which, as clustered groups, would be given very eccentric orbits. Such clustered groups of fragments are thought to constitute the nuclei of comets and from these gaseous emanations, developed by the heat of the sun, form comas and tails. Later, by dispersion, the fragments are scattered into meteorites.

This, in hasty terms, is the planetesimal conception of the origin of a family of planets, or rather planetoids, generated by centrifugal action from a spheroidal gaseous nebula while it is condensing into a sun. Such an origin belongs to the planetesimal genus of hypotheses in being strictly orbital, but the mode is distinctly different from that initiated by a passing star and evolved through the form of a spiral nebula.

Early in this paper it was intimated that the special problem of the planetesimal hypothesis was the origin and growth of the present planets only, but that the concept must, none the less, be in accord with the greater events of cosmic evolution. A few words on this point seem required to fill out our theme.

The origin of our sun, and of suns in general, was definitely set aside as not falling within the special problem for whose solution the planetesimal hypothesis was offered. The origin of suns must probably, none the less, be related, in some suggestive way, to the processes and principles on which the planetesimal hypothesis is based. It will have been noted that the planetesimal hypothesis postulates an oribtal state of the nebular matter. It is thus founded declaredly on orbital dynamics. In the phraseology of the naturalist, it belongs to the orbital genus. It is perhaps entitled to be regarded as the type of that genus, since it found its field by throwing down the doctrinal bars that shut off the orbital state from competency to give rise to forward rotations.

But suns are the great examples of gaseous bodies, and, as

such, are controlled by gaseous dynamics in contradistinction to orbital dynamics. There do not therefore seem to be obvious grounds for supposing that the stars came into being by an orbital process. Certain of the stars may indeed be concentrations of the large knots of the great spiral nebula, but these great knots are assigned to explosive separation from still greater gaseous bodies, and the real genesis of the gaseous state of the knots goes back to the origin of the great suns from which the nebulae are supposed to have sprung. The suggestion of Kapteyn ("Scientia," Vol. XIV., N. XXXII-6, pp. 345-357) that the irregular nebula of Orion has qualities that fit it for an initial place in the helium series of stars, seems to tally well with the view, entertained earlier in this paper, that center-to-center collisions of stars; or other massive bodies, would give rise to irregular or radiant nebulae. When one considers the prodigious velocities at which the great stars must collide, it is perhaps not too much to suspect that extreme dissociation attends the collision, and that this may be sufficient to give rise to the spectral effects which appear in the hydrogen-helium-nebulium nebulae, and that these may lead on to the spectral characters that appear in the helium stars and in the series that follows them. This speculative conjecture seems to find a measure of support in the succession of spectra observed in the Novae. If this be warranted, the origin of this class of stars falls into the final place in the series of stellar approaches, the supremely close approaches, the head-on collisions. This brings the whole into a series of harmonious relationships. The dynamics of the last term, however, are of the collision-rebound or gaseous type, not of the orbital. A center-to-center collision is, however, an event much less frequent than are such approaches as are held to give rise to spiral nebulae, and yet the stars greatly outnumber the known spiral nebulae. Perhaps a reconciliation of this discrepancy may be found in the probable fact that the stars endure longer than the nebulae, and in the multiplication of stars by spiraloidal separation into great knots that later condense into suns of a smaller order.

A further respect in which the stars seem to fall concordantly in with the orbital scheme of evolution, though not themselves of the orbital genus, seems to be disclosed by the recent discovery that the velocities of the stars are correlated with their spectral types, and these, in the opinion of some, are correlated with their ages, and in the opinion of others, with their masses. In either case, the relation is referred to the mutual influence of the stars in their approaches to another as time goes on, these approaches not being usually enough to call forth dispersive action. This class of approaches may be assigned a place at the distal end of the series of approaches, while the less distant order of approaches form the smaller spiral nebulae, the still nearer approaches the larger spirals, the penetrations of the Roche limit, the grazing approaches, and the glancing collisions, the still more effective members of the spiraloid series, while the center-to-center collisions form the extreme climax of the series, giving rise to irregular and radiant dispersions attended by extreme dissociation. The center-to-center collisions obviously destroy a large part of the motion of translation of the colliding stars by converting the energy of translatory motion into energy of dispersion. If, as suggested, the attendant dissociation develops the primoidal spectral state appropriate to a new stellar cycle, the coincidence is suggestively happy.

The evolution of suns is of a higher order than the evolution of planetary systems. The evolution of organized star clusters and stellar galaxies is of a still higher order. In these, suns are but the units. In the dynamic organization of star clusters and galaxies there is a return to the orbital realm. And here again, the fundamental agency may prove to be the Janus-faced function of gravity acting paradoxically and partitively as a dispersive as well as a centralizing agency. It has already been mentioned in passing that galaxies swinging near one another, or interpenetrating one another eccentrically, may develop spiraloid configurations on principles closely like those assigned to the origin of spiral nebulae. If this view is justified, or so far as it is justified,

our planetary hypothesis finds a correlative in an undeveloped galactic hypothesis.

It will perhaps be generally conceded that the energies that express themselves in orbital activities transcend those that express themselves in gaseous activities. In like measure, orbital cosmogony seems to transcend gaseous cosmogony in dynamical potency. But there are interchanges between them and no doubt an equilibrium between them. That both have fields whose immensity and intricacy transcend all human grasp goes without the saying.

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THE PARALLAX OF 61 CYGNI*

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TO find 61 Cygni with unaided sight, an observer must choose a time when the constellation is above the horizon mists and when the sky is not only clear but moonless. Even then it may be picked out only with care from among the neighboring faint stars against the faintly luminous background of the Milky Way. Yet in spite of its apparent insignificance, there is, for this double star, a long history of observation. It has been the object of frequent studies to determine its parallax, its proper motion and its characteristics as a binary system and in 1911 its name was given to a "moving cluster."[1]

Bradley at Greenwich[2] on September 25, 1753, was the first to record that the star was double. In 1792, Piazzi, when comparing his own observations with Bradley's, noted that the large proper motion indicated that this star was relatively near the earth.[3] This may be regarded as the beginning of the extended observations on its proper motion and to hold the germ of the many later determinations of its parallax. Observations of 61 Cygni were begun by Herschel[4] in September, 1780. He believed at that time that the two components of double stars were widely separated in space and began systematic cataloging of double stars and observations of the relative positions of the components in the hope that they might ultimately lead to the determination of annual parallax.[5].

In 1812 Arago and Mathieu determined at Paris[6] that the

* A portion of Contribution No. 29 of the Observatory of Columbia University.

[1] *Astronomical Journal*, Vol. 27, p. 33 and 67, B. Boss.

[2] Observed Transits of Sun, Planets and Fixed Stars over the Meridian, p. 129, Bradley.

[3] Specola Astronomica di Palermo, lib. VI, p. 10, note.

[4] *Philosophical Transactions*, Vol. 72, p. 138.

[5] *Phil. Trans.*, Vol. 72, p. 112.

[6] Annuaire, Bureau des Longitudes, 1834, p. 282. Note.

radius of the earth's orbit as seen from 61 Cygni subtends an angle less than 0″.5. Peters[1], using the data given by Arago, derives a value $\pi = + 0''.55$. This result is so near to later accepted values that it is a matter for real regret that no full description is available of the method by which it was obtained. This is specially true since Struve found from his investigations at Dorpat[2] between 1818 and 1821 that the quantities concerned in finding parallax were not within the measuring powers of the best existing instruments.

Peters[3] also publishes a value $\pi^1 = + 0''.47 \pm .51$, derived by him from the observations of von Lindenau at Seeburg in 1812-14.[4] Von Lindenau himself had stated that no parallax was indicated by the observations and the relation of the probable error to the value itself as given by Peters leads to no confidence in that result as significant.

Another unfruitful effort to find the parallax of 61 Cygni was made by Bessel[5] in 1815-16. He compared the position of the star with 6 others and was led to the values $\pi = - 0''.76$ and $\pi = - 1''.32$ from two series of observations.

In 1812, Bessel[6] made a speculative determination of the parallax of 61 Cygni from his measures of the distance between the two components. He assumed 400 years as the period of revolution and that the mass of the system equals the mass of the sun and derived the value $\pi = + 0''.46$.

When, in 1837, Bessel decided to make a new attack on the parallax problem with the aid of the heliometer which Fraunhofer[7] had constructed, he again chose 61 Cygni as the object

[1] Recherches Sur la Parallaxe des Étoiles Fixes. Mémoirs de L'Academie des Sciences, St. Petersburg, VI series, Vol. VII, p. 48.

[2] Beobachtungen der Universitäts Sternwarte, Dorpat, Vol. 3.

[3] Recherches Sur la Parallaxe des Étoiles Fixes. Mémoirs de L'Academie des Sciences, St. Petersburg, VI series, Vol. VII, p. 48.

[4] Table l'Ephemeride de Berlin pour 1818, p. 247.

[5] *Astr. Nach.*, Vol. 16, p. 71, also *Königsberger Beobachtungen*, Abth. II, p. VIII; Abth. III, p. IX.

[6] *Zach's Monatliche Correspondenz*, Vol. XXVI, 1812, p. 162.

[7] *Astr. Nach.*, No. 189.

for observation. The heliometer, or "divided object-glass micrometer," was first described by Dollond[1] in 1753. The Königsberg heliometer permitted more accurate measurements of angular distances than had been possible before. It was, then, partly to the perfection of his instrumental equipment that Bessel's success in determining stellar parallax is due.

Bessel[2] gives very clearly his reasons for choosing 61 Cygni as the first object for his investigation. The large proper motion suggested nearness and hence a relatively large, and possibly measurable, parallax. The star was in position for observation most of the year and there were many stars near it from which to select comparison stars. Bessel also was convinced that the image of the comparison star formed by the moving half of the object-glass could be brought to the point midway between the two components of a double star more accurately than it could be brought into coincidence with a single star image.

The values for the parallax of 61 Cygni resulting from Bessel's observations may be found in the accompanying Table. They constitute the first real knowledge of the scale of our stellar universe, and his result is not greatly modified by the latest values. The binary system of 61 Cygni is about 600,000 astronomical units, or 10 light years, from the solar system. Only 6 stars are known to be nearer.

Of the many determinations of the parallax of this star, since Bessel, some have been made like his in reference to the point midway between 61^1 and 61^2 Cygni, some for the individual stars. The reasons enumerated by Bessel for his choice of 61 Cygni are now supplemented by two others that have influenced succeeding investigators. The fame of the star as the first for which the annual parallax was determined lends a great weight, in itself, to making the decision to include it on any parallax program. More important, however, is the fact that it gives a standard for the

[1] *Phil. Trans.*, Vol. XLVIII, Part II, p. 551, An explanation of an instrument for measuring small angles.

[2] *Astr. Nach.*, Vol. 16, p. 70.

comparison of the excellence of each new series of parallax determinations. This was the reason that Pritchard employed the star for his first essay in the photographic determination of parallax. He writes[3] that the star "was selected on account of the unusually numerous examinations which have been applied to it by successive astronomers from the time of Bessel to the present date; my object being not so much to effect a redetermination of the parallax of this historical star as to obtain the means of comparing the photographic method with those other methods of micrometrical measurement heretofore directly applied to this end."

In No. 13 of the *Contributions from the Observatory of Columbia University* the parallaxes of 61^1 and 61^2 Cygni were determined by Davis from the Rutherfurd photographs. His Table XXIII gives the particulars of various determinations of the parallaxes of the separate components. No determinations derived from observations on the point midway between the two components were included. The list was published in 1898.

The accompanying Table includes determinations from the mean point and determinations since 1898.

In the column headed "parallax," π is used to indicate values of the parallax of the system regarded as a unit, π^1 and π^2 are used to indicate the values for 61^1 and 61^2 respectively.

[3] *Monthly Notices*, Vol. 47, p. 87 (1887).

Values of the Parallax of 61 Cygni

Method	Comparison Stars	Parallax	Probable Error	Authority	Year of Publication	Year of Observation
Königsberg, Heliometer	*a*	$\pi = +0''.3690$	$\pm 0''.0191$	Bessel	1838	1837-38
	b	$'' = .2605$	.0188			
	a and *b*	$'' = .3136$	.0136			
Königsberg, Heliometer	*a*	$'' = .3584$	.0131	Bessel	1840	1838-40
	b	$'' = .3289$	.0140			
	a and *b*	$'' = .3483$	.0095			
Königsberg, Heliometer	*a*	$'' = .3954$	.0138	Peters' Bessel	1849	1837-40
	b	$'' = .3424$	.0140			
	a and *b*	$'' = .3744$	.0099			
	a and *b*	$'' = .3602$ (Definitive)	.0121			
Königsberg, Heliometer	*a* and *b*	$'' = .351$	.020	Auwers' Bessel	1868	1837-38
	a and *b*	$'' = .530$	.028			1838-40
Poulkowa, Vertical Circle δ	Absolute	$''.1 = .43$		Glasenapp's Peters	1881	1842-43
Poulkowa, Vertical Circle δ	Absolute	$''.1 = .544$		Chandler's Peters	1893	1842-43
Poulkowa, Vertical Circle δ	Absolute	$''.1 = .413$	.084	Smirnow's Peters	1905	1842-43
Radcliffe, Heliometer	*c*	$'' = .360$	.025	Johnson	1853	1852-53
	d	$'' = .386$	.027			
	c and *d*	$'' = .430$	.026			
		$'' = .392$ (Mean)	.015			
	c	$'' = .392$	.030			
	d	$'' = .389$	.026			
	c and *d*	$'' = .427$	.016			
		$'' = .402$ (Mean)	.016			
Radcliffe, Heliometer	*c* and *d*	$'' = .511$	026	Auwers' Johnson	1863	1852-53
	c and *d*	$'' = .177$	.033			
Königsberg, Heliometer	38° 4355	$'' = .5588$ (Relative)	.0161	Auwers	1863	1860-62
	37° 4170	$\pi = .5638$ (Absolute)	.0162			

VALUES OF THE PARALLAX OF 61 CYGNI—(*Continued*)

Method	Comparison Stars	Parallax	Probable Error	Authority	Year of Publication	Year of Observation
		"	"			
Helsingfors, Photography (Donner)	4 stars	π^1 = +0·294		Kapteyn	1902	1891-92
		π^2 = ·348				
		" = ·321	± ·035			
		π_1 = ·304		Kapteyn (Veenstra)		
		π_2 = ·366				
		" = ·335				
		" = ·320	·035			
		(Mean)				
Göttingen, Heliometer	37° 4131	π_1 = ·332	·093	Peter's Schur	1903	1897
	38° 4405	π_2 = ·278	·013			
		π_1 = ·381	·084			
		π_2 = ·299	·027			
Poulkowa, Photography	59 stars	π_1 = ·31	·03	Kostinsky	1905	1896-98
	30 stars	π_1 = ·47	·03			
		π_1 = ·39	·02			
		(Mean)				
	59 stars	π_2 = ·38	·03			
	30 stars	π_2 = ·38	·03			
		π_2 = ·38	·02			
		(Mean)				
Heidelberg, Meridian Circle $\Delta\alpha$	Lal40652 W_2 20h 1762	π_1 = ·342	·061	Jost	1906	1899-1900
		π_1 = ·342	·060			
	Lal41131	π_1 = ·262	·049			
	D'Agelet 5760	π_1 = ·333	·036			
		π_1 = ·320	·029			
		(Mean)				
Cambridge Photography	10 stars	π_1 = ·406	·024	Russell	1910	1904·5
		π_2 = ·361	·021			
		" = ·384	·021			
Yale, Heliometer	1st pair of stars	π_1 = ·339	·010	Chase	1907	1904 6
		π_2 = ·295	·011			
	2nd pair of stars	π_1 = ·279	·010			
		π_2 = ·281	·013			
		" = ·291	·005			
		(Mean)				
Heidelberg, Meridian Circle $\Delta\alpha$	38° 4325	π_1 = ·24	·05	Abetti	1908	1906-7
	37° 4189	π_2 = ·22	·05			

VALUES OF THE PARALLAX OF 61 CYGNI—(*Continued*)

Method	Comparison Stars	Parallax	Probable Error	Authority	Year of Publication	Year of Observation
Cracow, Meridian Circle	38° 4323	$\pi^1 = +0''.276$	$\pm$ ·079	Jewdokimov	1912	1907·8
	37° 4189	$\pi^1 =$ ·291	·084			
		$\pi^1 =$ ·284 (Mean)	·071			
Upsala, Photography	4 stars	$\pi^1 =$ ·3431	·0085	Bergstrand	1903	1899-1903
		$\pi^1 =$ ·3404	·0176			
		$\pi^1 =$ ·3426 (Mean)	·0077			
		$\pi^1 =$ ·2804	·0119			
		$\pi^1 =$ ·3192	·0178			
		$\pi^1 =$ ·2923 (Mean)	·0091			
		π = ·2926	·0073			
Upsala, Photography	4 stars	$\pi^1 =$ ·375		Bohlin's Bergstrand	1908	1899-1903
		$\pi^1 =$ ·337				
		$\pi^1 =$ ·363	·010			
Leipzig, Heliometer	37° 4131	$\pi^1 =$ ·278	·023	Peter	1903	1901·2
		$\pi^2 =$ ·291	·014			
	38° 4405	$\pi^1 =$ ·254	·026			
		$\pi^2 =$ ·290	·012			
Königsberg $\Delta\delta$	7 stars	$\pi^1 =$ ·265	·024	Struve	1914	1902·4
		$\pi^1 =$ ·327	·015			
		π = ·309	·013			
Yerkes, Photography	4 stars	$\pi^1 =$ ·267	·011	Hopkins	1915	1911·13
		$\pi^2 =$ ·277	·012			
	8 stars	$\pi^1 =$ ·256	·007			
		$\pi^2 =$ ·251	·012			
Yerkes, Photography	6 stars	$\pi^1 =$ ·267	·004	Slocum	1914	1911·13
		$\pi^2 =$ ·277	·006			
Sproul, Photography	16 stars	$\pi =$ ·301	·010	Miller	1914	1912·13

NOTES FROM THE METEOROLOGICAL SERVICE

Summary Report of the Weather in Canada

September, 1916.

Temperature.—The temperature nowhere differed much from the average. Over Alberta and the extreme western part of Saskatchewan, it was from average to 2° above; in Quebec and in nearly all portions of the Maritime Provinces it was also from average to 2° above, whereas over the large remaining portion of the Dominion, it was from average to 2° below.

Precipitation.—In British Columbia the rainfall was above the average general record throughout the northern portion of the Province, elsewhere it was below. In the Western Provinces it was above the average, except locally, especially in Northern Alberta and in the Qu'Appelle Valley. In Ontario the average was well exceeded, except over the country to the southward of the Georgian Bay district, and eastward to Prince Edward County. In Quebec it was more than the average throughout the Province, whereas in the Maritime Provinces it was everywhere below the average. Some of the chief negative departures were Vancouver 2.23 inches; Charlottetown, 2.20 inches; St. John, 1.82 inches; Yarmouth, 1.60 inches; Halifax, 1.31 inches; Toronto, 1.19 inches; and some of the most pronounced positive departures were Barkerville, 1.30 inches; Edmonton, 1.33; inches; Qu'Appelle, 3.67 inches; Port Arthur, 1.90 inches; Coldwater, 2.50 inches; Brome, 2.90 inches; Quebec, 1.35 inches.

Temperature for September, 1916

Station	September		Station	September	
	Highest	Lowest		Highest	Lowest
Yukon			Kenora	76	27
Dawson	72	26	Kimmount	83	27
British Columbia			Kingston	80	33
Atlin			Kitchener	88	32
Agassiz	83	35	London	90	29
Barkerville	69	22	Lucknow	85	27
Kamloops	81	33	Markdale	87	31
New Westminster	80	35	North Gower	85	25
Prince Rupert	78	37	Oshawa	82	25
Vancouver	72	39	Ottawa	83	35
Victoria	80	43	Paris	88	30
Western Provinces			Parry Sound	84	25
Battleford	81	23	Peterboro'	86	30
Calgary	80	23	Port Arthur	76	26
Edmonton	78	28	Port Burwell		
Medicine Hat	84	28	Port Dover	82	35
Minnedosa	80	24	Port Stanley	79	32
Moose Jaw	85	24	Queensborough	84	23
Oakbank	77	28	Ronville	81	28
Portage la Prairie	80	27	Southampton	86	36
Prince Albert	80	26	Sundridge		
Qu'Appelle	80	24	Stonecliffe	82	30
Regina	80	26	Stony Creek	92	36
Saskatoon	77	24	Toronto	90	35
Souris			Uxbridge	84	30
Swift Current	84	24	Wallaceburg	89	30
Winnipeg	78	28	Welland	86	31
Ontario			White River	74	18
Agincourt	90	30	*Quebec*		
Aurora	85	26	Brome	80	31
Bancroft	82	24	Father Point	78	36
Barrie	87	31	Montreal	78	30
Beatrice	81	25	Quebec	76	37
Bloomfield	85	33	Sherbrooke	80	36
Brantford			*Maritime Provinces*		
Chapleau	76	28	Charlottetown	78	40
Chatham	95	33	Chatham	84	34
Clinton	85	29	Dalhousie	75	35
Collingwood	84	35	Fredericton	83	34
Cottam			Halifax	80	38
Georgetown	87	31	Moncton		
Goderich	84	36	St. John	80	40
Gravenhurst			Sussex	79	32
Grimsby	90	37	Sydney	82	38
Guelph	85	27	Yarmouth	76	37
Haliburton	83	29			
Huntsville	83	26			

MAGNETIC OBSERVATIONS

August, 1916

During the month of August the magnetic forces were particularly active. Large disturbances were recorded at Agincourt on the 2nd, 6th, 22nd, 26th and 27th. That on the last mentioned date was synchronous with a beautiful auroral display, which was reported from all parts of Canada. It began very suddenly about $14^h\ 42^m$ of the 26th and displayed great activity between 17^h of the 26th and 2^h of the 27th, passing beyond the limits of the photographic paper on all three traces. Reproductions of the records of this storm are given, together with the record obtained at Meanook, Alta., of Declination.

The disturbance of the 22nd also began very suddenly about $13^h\ 26^m$ of the 22nd on all three curves, and continued moderately active until about 4^h of the 23rd. The greatest movements were recorded in H and Z, with maximum increase of force about $15^h\ 26^m$. H reached about 257γ and Z about 234γ above normal. From commencement up to 19^h the forces were above normal, and from 19^h of 22nd to the end, slightly below.

The disturbances on the 2nd and 6th were both of less intensity than the others, but some moderately large departures from normal were recorded. The H and Z forces were generally below normal, whilst the D movements were both above and below.

At Meanook, Alta., the Declination record indicates general conditions, very similar to those at Agincourt. The large disturbances were recorded on the same dates, and, in the case of those with sudden beginnings, the absolute time of commencement was practically the same.

AGINCOURT MAGNETIC OBSERVATIONS

August, 1916		D. West	H	Z	I
	Mean of Month	° ′ 6 33·6	0·15978	0·58510	° ′ 74 43·6
	Maximum	*8 24·2	*0·16328	0·58749	
	Date of Maximum	27	26	22	
	Minimum	5 5·7	*0·15439	*0·58085	
	Date of Minimum	26	27	26	
	Monthly Range	3 18·5	0·00889	*0·00664	
Mean Daily	From hourly readings	0 14·8	0·00061	0·00027	
Amplitude	From means of extremes	0 33·2	0·00139	0·00088	

H and Z are expressed in C.G.S. units (1 γ = 0·00001 C.G.S.)

All results are reduced to International Magnetic Standard.

*Values of Maximum Westerly Declination, Maximum and Minimum Horizontal Force and Minimum Vertical Force are estimated. In each case the light passed off the photographic paper

W. E. W. J.

MAGNETIC OBSERVATIONS

Meanook Observatory

In July, 1916, a magnetic observatory was established near Meanook, Alberta, for the purpose of securing photographic records of the changes in Declination or variation of the compass.

The photographic Declinometer consists of a cylindrical steel magnet suspended by a phosphor bronze ribbon to a torsion head graduated to half degrees. Below the magnet is attached a small mirror, which reflects a ray of light on to sensitised paper placed on a drum which is rotated once in about 26 hours by means of a clock drive, and which is placed at suitable distance from the mirror to get the magnification required. A second mirror is placed immediately below this mirror, but attached to the base of the instrument, and so kept permanently in a fixed position with relation to the direction of the ray of light, and thus serves the purpose of giving a base line on the photographic paper from

which the variations in the line traced by the movable mirror may be determined. A master clock is provided, in which an electric contact has been constructed that closes an electric circuit at each hour and by means of an electro-magnet operates a shutter to cut off the light from the sensitive paper for a period of about 3 minutes, thus giving a time mark on the photographic trace. The value of 1^{mm} ordinate was determined to be 1′.39 of arc on July 21, 1916, and on July 22 the value of the base line was determined from 10 observations with magnetometer 15 to be 28° 45′.3 East of North, and increase of ordinate signifies decrease of Easterly Declination.

On July 10 and July 12 the latitude of the station was calculated from meridian observations of the sun, the resulting value being 54° 36′.9 N., and on July 10, 12 and 23 the longitude from morning and afternoon observations of the sun, using time by Government telegraph and chronometer, was determined to be 113° 20′.5 W. of Greenwich.

Absolute observations were made for H and I, with the following results:—

Date	105th M.T.	H	105th M.T.	I
				° ′
July 15	10 32	0·12914		
July 17			17 02	77 52·6
July 19			10 45	77 54·8
July 23	17 54	0·12974		

Observations for H with Magnetometer 15 reduced to International Magnetic Standard.

Observations for I with Dover Dip Circle 200 uncorrected.

MEANOOK MAGNETIC OBSERVATIONS

August, 1916		D. East	I
		° ′	° ′
	Mean of Month	27 48·6	77 52·9
	Maximum	29 7·6	
	Date	26	
	Minimum	25 28·8	
	Date	26	
	Monthly Range	3 38·8	
Mean Daily Amplitude	From hourly readings	0 15·2	
	From means of extremes	0 47·1	

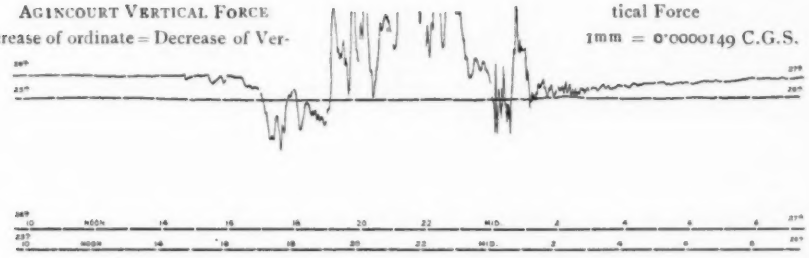
Declination observations reduced to International Magnetic Standard. Those for I are the mean of all observations made during the month with Dover Dip Circle No. 200.

W. E. W. J.

AGINCOURT VERTICAL FORCE

Increase of ordinate = Decrease of Vertical Force

1mm = 0·0000149 C.G.S.

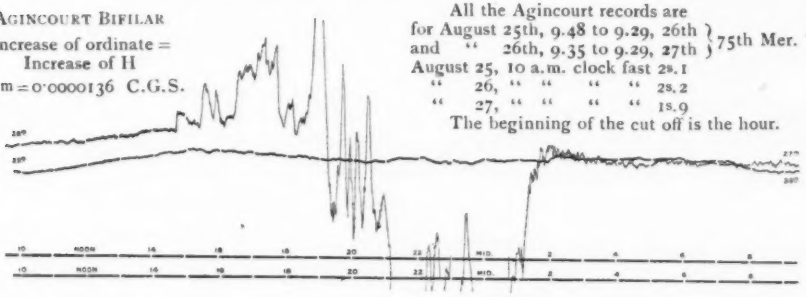


AGINCOURT BIFILAR

Increase of ordinate = Increase of H

1mm = 0·0000136 C.G.S.

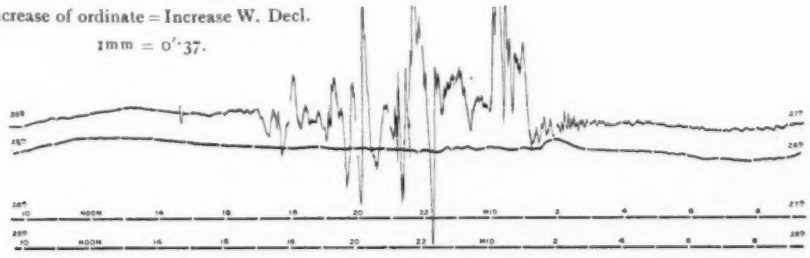
All the Agincourt records are for August 25th, 9.48 to 9.29, 26th } 75th Mer. T.
and " 26th, 9.35 to 9.29, 27th }
August 25, 10 a.m. clock fast 28.1
" 26, " " " " 28.2
" 27, " " " " 18.9
The beginning of the cut off is the hour.



AGINCOURT DECLINATION

Increase of ordinate = Increase W. Decl.

1mm = 0'·37.

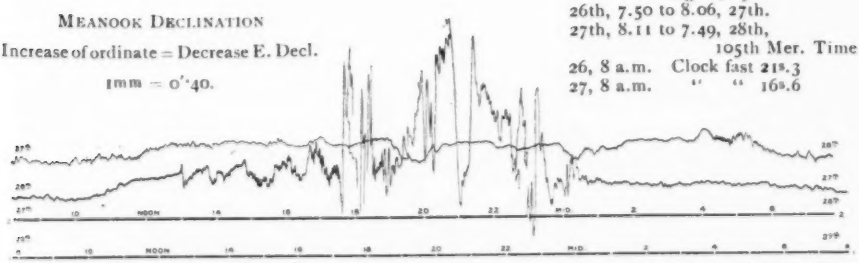


MEANOOK DECLINATION

Increase of ordinate = Decrease E. Decl.

1mm = 0'·40.

August, 1916
26th, 7.50 to 8.06, 27th.
27th, 8.11 to 7.49, 28th,
105th Mer. Time
26, 8 a.m. Clock fast 21s.3
27, 8 a.m. " " 16s.6



RECORDS OF THE MAGNETIC ELEMENTS AT AGINCOURT (NEAR TORONTO) AND MEANOOK, ALBERTA, DURING THE GREAT AURORAL DISPLAY, AUGUST 26, 1916

Earthquake Records by the Milne Seismograph

TORONTO

Sir Frederic Stupart, Director.

P.T. = Preliminary Tremors. L.W. = Large Waves. A.C. = Air Currents.
Time is Greenwich Civil Mean Time, 0 or 24 h = midnight.

No.	Date 1916	P. T. Comm.	S. Comm.	L.W. Comm.	Max.	End	Max. Amp.	Remarks
		h m	h m	h m	h m	h m	mm.	
1631	Sept. 3			8 9·6) 8 16·7)	8 28·2	9 43·3	1·5	
1632	" 3			10 52·2		10 59·0	0·1	
1633	" 5			23 13·9	23 30·8	0 03·0	0·8	
1634	" 6			0 21·0?		9 35·4	0·05	
1635	" 11			6 59·2		8 27·9	0·2	
1636	" 15		7 25·7?	7 36·0) 7 48·3)	8 00·8	8 48·8	1·0	S. Waves doubtful.
1637	" 21			19 05·3			0·05	Time doubtful. No cut-off.
1638	" 23	5 48 7?	5 55·8	5 58·6) 6 01·0)	6 05·1	7 01·5	1·8	5340 km.?
1639	" 24			18 00·0		18 05·0	0·05	
1640	" 28			12 03·7		12 11·5	0·2	
1641	" 29			19 21·4		19 50·0	0·1	

Period of boom 18 seconds. Pillar inclination 0°·50.

VICTORIA, B. C.

F. N. DENISON, SUPERINTENDENT.

No.	Date 1910	P.T. Comm.	S. Comm.	L.W. Comm.	Max.	End	Max. Amp.	Remarks
		h m	h m	h m	h m	h m	mm.	
1632	Sept. 3	7 37·8	7 42·7	7 50·7?	8 04·1	8 43·7	1·5	3,220 km.
1633	" 3	9 41·9			9 43·8	9 49·7	0·1	{ May be part of long
1634	" 3	10 23·9		10 26·8?	10 31·8	10 35·8	0·2	{ distance quake.
1635	" 5	21 50·1?			21 55·6	22 01·0	0·1	
1636	" 5	22 38·7	22 43·6	22 56·5	23 02·5	23 37·2	0·5	3,330 km.
1637	" 6	8 50·5		8 55·5	9 00·0	9 16·3	0·1	
1638	" 11	6 58·4	7 06·3	7 13·7	7 33·6	8 40·5	0·3	?6,360 km.
1639	" 15	7 10 2	7 19·1	7 31·0	7 45·9	8 32·5	0·2	?7,530 km.
1640	" 19	11 05·8		11 06·8	11 07·8	11 09·3	0·2	550 km.
1641	" 21	18 56·3		18 57·7	18 59·2	19 01·2	0·3	820 km.
1642	" 23	5 56·7	6 02·2	6 08·3	6 19·5	6 53·7	1·0	?3670 km. May be off
1643	" 24	17 42·0?			17 43·2	17 44·0	0·2	{ We s coast Mexico.
1644	" 24	19 37·7		19 38·7	19 39·7	19 42·2	0·3	?550 km.
1645	" 25	2 10·6			2 11·6	2 13·1	0·2	
1646	" 26	15 52·6			15 53·1	15 54·6	0·1	
1647	" 26				22 17·6?		0·05?	Doubtful quake.
1648	" 28	P. or L. W.	11 43·7		11 47·2	11 53·1	0·2	
1649	" 29	19 29·0	19 32·1	19 34·0	19 37·0	19 43·5	0·5	1,810 km.

Boom period 18 seconds. Pillar inclination 0″·54

J. V.

ASTRONOMICAL NOTES

COLOR OF NEBULÆ.—Important differences in the spectra of gaseous nebulæ, even those of the same general class, have been known for many years. Although the characteristic nebular lines λ 5007 and λ 4959 seem to maintain a constant ratio of brightness, their behavior varies greatly with respect to *Hβ*, λ 4686, λ 3727, and other lines. Thus *Hβ* is usually fainter than the chief nebular line λ 5007, but there is a wide range in the relative intensities, and intensity reversals may even occur. Again, lines usually present are as yet unobserved in certain objects, and in some we have apparently only the monochromatic radiation λ 3727.

This diversity is reminiscent of the differences in the spectra of stars. These are universally accepted as evidence of evolutionary change, and if nebulæ have any part in the scheme of development, it is likely that their spectral peculiarities will find a similar interpretation. In this event spectral correlations must exist, more or less clearly defined according to the development of the nebulæ compared. The important investigations of Wright have already revealed relationships presumably of this kind.

Further, experience on other fields suggests the probability of spectrum differences within the same nebula.

Lack of homogeneity has already been observed in numerous nebulæ; but probably the phenomenon is more nearly universal than has hitherto been supposed, and one naturally inquires whether internal differences may not provide useful criteria of development supplementing those based on spectral variations from nebula to nebula. In fact Wright has already called attention, in the case of planetary nebulæ, to the gradually increasing concentration of λ 4686 within the nucleus as we approach the Wolf-Rayet stars in his graded series of spectra. He has made

this circumstance a basis for classification, and it seems likely that such criteria will be even more used in the future.

The usefulness of monochromatic photographs seems thus to have been demonstrated, but it does not appear that they have been employed except for the Orion Nebula. A wider application has therefore seemed desirable, particularly for objects too faint for detailed spectroscopic observation. Among these one naturally turns to the spiral nebulæ because of the unexpected result uniformly found for the central region, namely, an absorption spectrum of intermediate or late type. Even admitting the presence of superimposed bright lines, which has in some cases been claimed, the result is still remarkable for objects whose finer details are suggestive of some of the gaseous nebulæ.

Three of the well-known spirals, Messier 51, 94, and 99.

Few if any of the yellow images are smaller than the corresponding blue images, at least for objects which are certainly stars; but this is by no means the case for the condensations and nebulosity comprising the branches of the spirals. For these the relative weakness of the yellow light is a striking feature for each of the objects mentioned. Quantitative measures of intensity have not yet been made, but there is every reason to believe that the nebular condensations will reveal negative color indices of large amount. The knots of nebulosity are certainly bluer than the bluest of the neighboring stars, and one is reminded of the great photographic activity of the central star in the Ring Nebula in Lyra.

Whatever the spectral character of these outlying regions may prove eventually to be, it must differ from that of the central nucleus, for the three central nuclei and the secondary nucleus at the end of one of the branches of M 51 are all much stronger in yellow light than in blue. Here the color seems to be in accordance with the typical absorption spectrum found in all similar objects thus far observed.

It is still too early for any general conclusion, but preliminary photographs of other spirals suggest similar results; and it seems not unlikely that the phenomena described are typical of this class of objects.

In contrast to the spirals it is of interest to note the results for the bright planetary N.G.C. 3242. In this instance no important differences are revealed by the blue and yellow exposures, at least none which cannot be accounted for by possible differences in gradation on the two kinds of plates.—FREDERICK H. SEARES, Mount Wilson Solar Observatory, in *Proceedings Natural Academy of Sciences*, October 15, 1916.

MONOPHOTOGRAPHS OF SATURN AND JUPITER.—In the *Astrophysical Journal*, Vol. XXXVI. p. 75, 1912, I gave an account of the results obtained by photographing the moon through ray-filters transmitting limited regions of the spectrum. Marked differences in the distribution of light and shade were observed in the different pictures, the most conspicuous being an extensive dark deposit around the crater Aristarchus, which appeared in the picture made by means of ultra-violet light but was quite absent in the one made by the yellow rays. Laboratory experiments on the appearance of various rocks and minerals, when photographed through these same ray-filters, made it appear probable that the dark spot was due to sulphur or some sulphur-bearing rock.

It seemed quite likely that results of even greater interest would be obtained if the method of monochromatic photography were applied in the case of planets, but instrumental facilities were not at my disposal at the time.

Photographs were made of the moon and Jupiter with infra-red, yellow, violet and ultra-violet light, and the times of exposure, with the screen, were determined for both silvered and nickeled mirrors. As a result of this preliminary work, I was able to get satisfactory results at once with the 60-inch reflector of the Mount Wilson Observatory, which was placed at my disposal for four nights in the latter part of October.

With this instrument a very complete set of photographs was made of the moon, Jupiter and Saturn, by light of the four regions of the spectrum referred to above. Results of very great interest were obtained in the case of the two planets, for dark

belts were shown by violet and ultra-violet light, of which no trace appeared visually or on the plates made by yellow light, while the infra-red pictures showed scarcely any trace of the belts ordinarily seen. By applying the methods of three-color printing to these plates we can produce a colored positive, in which the belts of selectively absorbing gases appear strongly colored ; in other words, we produce an effect somewhat similar to what we should see if our eyes were sensitive to a wider spectral range.

The most interesting results were obtained in the case of Saturn. The picture taken through the infra-red screen showed the ball of the planet practically devoid of surface markings, there being only the merest trace of the belts ordinarily seen. Through the yellow screen the planet presented its usual visual appearance, the narrow belts showing distinctly. On the plates made with the violet ray-filter (transmission $\lambda 4000$–$\lambda 4500$) a very broad dark belt surrounded the planet's equator, occupying the region of the planet which was brightest in yellow light. In addition to this dark equatorial belt, a dark polar cap of considerable size appeared in the pictures. So different were the two pictures that, were it not for the ring, it would have been difficult to believe that they represented the same object. In ultra-violet light the appearance was much the same, but the dark belt was not quite as wide, the bright region between the polar cap and the belt being distinctly broader.

Two hypothesis suggest themselves in explanation of the dark belt. We may be dealing with a fine mist or dust which forms an extension of the crape ring down to the ball of the planet. This hypothesis appears to be favored by the circumstance that, on the negatives made by the violet and ultra-violet light, the sky between the ball of the planet and the ring is distinctly denser than the region just outside. This would indicate that the region inside of the ring is filled with some material which reflects the short wave-lengths to a slight degree. No trace of this darkening appears on any of the plates made with the yellow screen, even on one that was many times overexposed,

which appears to show that the phenomenon is real. This peculiarity was, however, not detected until the work was finished, and I should prefer to verify it or have it verified before recording it as an established fact. The luminosity is much too feeble to show in the prints.

The second hypothesis assumes the existence in the planet's atmosphere of some substance capable of absorbing violet and ultra-violet light. This material might be a fine mist or dust, or some gas capable of absorbing the more refrangible part of the spectrum. Such a gas would be of a pale yellow color, and sulphur vapor and chlorine naturally occur to us. I have, however, examined the absorption spectrum of both of these gases in quartz bulbs, with the result that the absorption appears to be much stronger in the ultra-violet than in the violet, which is in disagreement with the circumstance that the band appears wider in violet than in ultra-violet light.

Photographs of Jupiter made with the four ray-filters showed differences almost as marked as those found in the case of Saturn, though not so striking by casual inspection. The rotation of the planet is so rapid that it was found necessary to make the series of pictures as quickly as possible, if comparable results were to be obtained. Following was accomplished by bringing one of the outer satellites on the cross-hair of the eyepiece in the movable plate-holder. The exposure times were two seconds for the violet and yellow negatives, seven seconds for the ultra-violet, and ninety seconds for the infra-red. The dark belts are scarcely visible on the infra-red plates, while the pictures made with violet light show them in the greatest contrast.—R. W. Wood, *Contributions*, Mount Wilson Solar Observatory, No. 113.

Apex and Velocity of Solar Motion.— In a paper on the residual radial velocities of the stars of the different spectral classes contained in Campbell's well-known catalogues of about 1300 of the brighter stars, I pointed out several peculiarities, among which were large and apparently systematic residuals in the regions at right angles to the axis of solar motion.

In attempting to find the cause of the apparently systematic discordance of nearly 10° between the declinations of the solar apex as derived from radial velocities and proper motions, the first step was a determination of the apex from the proper motions of the stars of Class B. It was at once seen that there was a marked discordance between the mean proper motions of the groups in the northern and southern hemispheres. This discordance was so marked and so suggestive that solutions were made for the apex and velocity of solar motion from the radial velocities of the northern and southern B stars separately. The results of these solutions and similar ones for some other classes yielded widely different positions of the solar apex.

At this point I received a letter from my friend, Professor Kapteyn, to whom I had written regarding the peculiarities found, in which he suggested that a different apex for the solar motion would remove the systematic residuals found in the regions of 0^h and 12^h of right ascension. This appears to be essentially the case. But I was now convinced that no single position of the apex would satisfy all of the discordances which I had found, not only from the radial velocities, but from the proper motions of the B stars as well. These discordances were believed to be something more than merely accidental. Separate solutions were made from the radial velocities of the north and south stars of all of the spectral classes. The separation into north and south groups divides the effect observed in the B stars on opposite sides of the axis of solar motion sufficiently at least for a preliminary investigation.

Discussion of these results is reserved until investigation have been made of other conditions which should also bear upon an explanation, such as proper motions, distances, the distributions of residual velocities and of the relations which appear to exist among the different conclusions themselves. The general conclusion may be stated, however, that the apparently systematic differences in the position of the apex and in the velocity of the solar motion strongly indicate, if they do not establish, variations of the general direction and velocity of motion of the stars themselves in different portions of our stellar system.—C. D. PERRINE, *Astrophysical Journal*, No. 2, 1916. J. R. C.

NOTES AND QUERIES

Communications are Invited, Especially from Amateurs. The Editor will try to Secure Answers to Queries.

Query

Mr. Andrew Elvins sends the following query, which was a long time in reaching me :

In the April Journal, referring to a 'home-made' aurora, you are led to conclude that what appeared like an aurora was volatilized particles of copper. The result was a good imitation of auroral streamers.

Will not this experiment help us in our search for the cause of auroras? If sun-spots are caused by the inrush of cosmic matter into the sun, as my "Theory of Aggregation" (1868) contends, volatilization will be the result of the sun's heat. Copper will sometimes fall, and the same will follow as you have described.

Answer

There can be little doubt that masses of matter are continually falling into the sun, and that copper must sometimes be included in that matter. It would certainly be volatilized, and would produce green flashes in the solar atmosphere, but it is not easy to see how they would reveal themselves in greenish streamers in our atmosphere. The aurora is probably caused by something which comes from the sun,—corpuscles, electrons, or something else—but it can hardly be as *material* as the particles of copper seen in the artificial aurora.

Query

The following query, from a member of the Victoria Centre now serving in No. 6 Co. R.C.G.A., St. Lucia, B.W.I., was delayed through the Editor's absence from home.

I thought that the following might possibly be of interest to readers of the JOURNAL. It is from Coleridge's "Ancient Mariner" and is given in the marginal references of Edgar Allen Poe.

"All in a hot and copper sky
The bloody sun at noon
Just up above the mast did stand,
No bigger than the moon."

Is the apparent diameter of the sun greater than that of the moon? I think we might also ask, Is it possible that Poe did not know the difference between a *total* and an *annular* eclipse of the sun?—T. T. W.

ANSWER

According to the "American Ephemeris" the apparent diameter of the sun at mean distance is 31′ 59″·26; that of the moon, 31′ 5″·16. While the former is greater than the latter it could not possibly be detected by naked-eye observations. I think, too, that Coleridge did not have in mind any scientific fact; his great weakness in such matters is strikingly shown in the well-known couplet in the same poem:

"The horned moon, with one bright star
Within the nether tip."

The real explanation of the stanza seems to me to be as follows. If an object is intensely bright we are deceived as to its size. In the arc lamp the intense source of the light is very small, often not half-an-inch in diameter, but most people would take it to be a foot or more across. Similarly, in a clear sky the glowing sun appears very much larger than the faint moon does. However, if the air is thick with smoke or haze the intense glare is removed, while the disc is blood-red and apparently much smaller.

In regard to Poe's scientific attainments I am not well informed. Perhaps some reader will answer the question.

MASS AND WEIGHT

Dr. Otto Klotz, of Ottawa, writes as follows:—

In the note on gravitation in the October number of the

JOURNAL (page 463). I find Mr. Whitmell a little confusing with mass and weight, using the latter when he means the former. His very first statement is, perhaps, a little obscure or ambiguous for a beginner. The dimensions of G are $M^{-1} L^{3} T^{-2}$, derived from,

$$F = G \frac{m m'}{r^2} \text{ or } M L T^{-2} = M^2 L^{-2} G.$$

F is dynes but not G.

The only text book on physics that I know that consistently uses mass and weight discriminately is that of Reed and Guthe.

A GREAT MAGNETIC DISTURBANCE

In the notes on the great auroral display of August 26 printed in our last number Dr. Satterly stated that on inquiry at the Meteorological Office he learned that there was a decided magnetic storm at the same time. In the present number (page 511) are reproduced the photographic records made at Agincourt, near Toronto, and at the automatic recording magnetic station recently established at Meanook, in northern Alberta. This work is in charge of Mr. W. E. W. Jackson, who informs me that the record shows the disturbance to have been, as far as can be judged, absolutely simultaneous at the widely separated stations. Indeed this is seen quite well on the reproduction. It will be remembered, too, that Mr. Collier first saw the display at sunset, while in Ontario it was seen from about eight o'clock.

Considerable interest has been aroused by this display and in the next issue further reference will be made to it.

C. A. C.